stratosphere seems to be not constructive, but conservative and registrative. It protects the energy from being dissipated by "filling up," because the descent of its isothermal air is arrested by the adiabatic rise of temperature. That is, indeed, the common function of all "decks" or lids in the atmosphere, of which the stratosphere is the chief. At the same time, for an observer the stratosphere registers the locality of low pressure by the lowness of the tropopause and the relative warmth of the air column above it. It seems to be a law for the general circulation and for local circulations that as pressure diminishes in the troposphere the tropopause is lowered and the temperature of the columns above it rises.

Consequently, my view at the present time is that the energy of a cyclone is due originally to convection in a region with a suitable law of variation of velocity with height; it is guarded at the top by the isothermal condition of the stratosphere, and on the sides by the balance of pressure and rotation. It is open to slow attack at the bottom on account of the friction of its winds with the surface, and unless its energy can be maintained by additional convection it must perish. I do not think that a traveling cyclone carries its supply of rain for long distances; it probably manufactures it out of the material in the lowest levels which it has to pass over. But it uses the energy so supplied first to form a secondary, and afterwards to absorb it or to be absorbed by it.—*Napier Shaw.*

It is a well-known hydrodynamical result that, in the absence of any external stabilizing influence, any surface of discontinuity of velocity in a fluid must be unstable. The effect of this instability is seen in the eddies produced in a mill pond, at the margin of the entering stream. A sufficiently rapid shearing, without actual discontinuity, will produce the same effect. Most atmospheric eddies are developed in this way. In the case of differences of velocity between different masses of air at the same level, gravity is not directly available to damp any eddies that may be produced, and hence it does not seem likely to be difficult to account for eddies with their axes vertical.

Thus the origin of cyclones may well be explained on the lines suggested in Mr. W. H. Dines's letter in *Nature* of November 18. It is rather more difficult to see what determines the size and intensity to which they grow. Ground friction must play its part; also, where the warm stream on the south side bulges northward, it must do so to some extent over the top of the cold air already there, and this arrangement makes for stability, and when sufficiently developed must prevent the further growth of the disturbance.

The speed of translation of the cyclone on this theory should be the mean of the velocities of the two currents, which is usually about correct. The geostrophic condition must also hold approximately, otherwise the disturbance would spread out with nearly the velocity of sound and disappear. What is not easy to see, however, is why the isobars tend to become more or less circular instead of wavy.—*Harold Jeffreys.*

I should like to express my agreement with Mr. W. H. Dines's view (*Nature*, Nov. 18, p. 375) regarding the origin of the initial difference of pressure which leads to the development, under the influence of the earth's rotation, of cyclonic circulation, and to state that I have often suggested that this initial disturbance may have a mechanical origin (see Quart. Journ. Roy. Meteor. Soc., vol. xliii, 1917, p. 27). At the same time it seems that one can not, on many grounds, ignore the effect of temperature contracts as a contributing factor in the further development and maintenance of storm energy.

To take the very fact which Mr. Dines cites, namely, the exceptional storminess of the Atlantic to the northwest of Scotland. This region is, in a most conspicuous degree, stormier in the winter months than in the summer, and it is almost one of the canons of physical geography that the excessive development of storm energy during the cold season is favored by the great contrast in temperature between the frost-bound continents and the warm Atlantic, the individual cyclonic systems breeding not so actively over the land areas, where the general pressure is high, as over the oceanic areas, where the general pressure is low. On the other hand, during the warm season—when the temperature gradient between the oceans and the continents is reversed, but is much less steep than the winter gradient—cyclonic energy in the North Atlantic is far less powerful, while over the sun-heated continents storm energy takes the form, not of extensive wind systems, but of localized convectional thunder systems. Furthermore, in the southern ocean, between 40° and 60° S., where there are no disturbing land masses, there does not appear, judging from the reports of navigators, to be such conspicuous seasonal difference in storminess, and this is borne out by statistics available for the Falkland Islands (Meteor. Office Geophys. Mem., No. 15).—*L. C. W. Bonacina.*

A rebuttal by Mr. Deeley is published in *Nature*, December 16, 1920, page 502, and adverse comment on it by W. H. Dines in the issue for December 23, page 534. The second paragraph of Mr. Dines's letter relates to the earlier comments of Sir Oliver Lodge:

> The point mentioned by Sir Oliver Lodge in his letter in *Nature* of November 25, has been, I think, put forward by von Bezold and others, but Sir Oliver seems to have overlooked the result of the heat set free by the condensation of the vapor. Could a cubic meter of damp air be confined in an adiabatic but extensible balloon and the vapor be condensed by any means, the result would be an increase of volume, for the expansion due to the heat produced by the condensation would far more than balance the contraction due to loss of pressure. If, indeed, the heat energy due to the latent heat of vapor all took the form of kinetic energy in the atmosphere, quite a trifling rainfall would suffice to produce over the same area the most violent cyclone ever recorded.

Prof. Alexander McAdie having become particularly interested in Sir Napier Shaw's discussion, wrote to him and obtained further comments. At the close of a discussion [2] of Shaw's published letter (quoted above), Prof. McAdie says:

> Finally, one further proposition appears to Shaw to be worth study, namely, that "an anticyclone is a region of descending air if the month is the unit of time; but if the unit of time is the hour or day, an anticyclone is simply an unchanging mass except for the outflow at the bottom." This outflow he has calculated (in a letter to the writer) as a settlement at the rate of 100 meters per day, about 0.7 m/s. In a cyclone per contra, the air rises at such a rate that the hour and the day are units of time.
>
> This conception of the time factor is novel, and if we permit its introduction, as it seems we must, then we are faced with the further problem of determining the life histories of slow-moving anticyclonic air descending from heights of 8 or 10 kilometers, most leisurely, requiring 6, 8, or 10 days to make the downward passage and landing far, far away from the place of setting out. In the case of hyperbars [large subpermanent high-pressure areas], such as the Atlantic [or Azores] anticyclone, Shaw says: "The out curvature is everywhere *the same angle;* and the velocity is proportional to the distance from the center. If $V=Cr$ and the velocity normal to the circle is proportional to r, the outward velocity will be $C'r$ and hence the outflow $\alpha 2\pi r^2$ is proportional to the area; and therefore implies a uniform settlement all over the area."

Much more will be said about cyclones and anticlones and the general circulation of the atmosphere when we have free-air data from more than a few small sections of the vast regions traversed by moving low and high pressure areas.—*C. F. B.*

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THE RAPID FALL IN TEMPERATURE OF COLD WAVES.

A high pressure area will sometimes form over night, especially in or over the Lake region, where the temperature the day before was about normal for the season all over the district. Immediately the temperature falls with the accompanying northerly winds perhaps 20° or even 40° [F.] in 12 or more hours. This is quite a common occurrence in this section [northern New York] in the winter. This air could not have had time to lose so much heat by *radiation*, neither could the wind at the surface have had time to *import* so low a temperature. There are many clear nights with high barometer, with every opportunity for radiation to cause a big drop in temperature, but no such fall is observed other than perhaps 10° F. Last night [Jan. 26–27, 1921] under a clear sky and high pressure the temperature actually rose several degrees and without wind movement. A perfect calm prevailed. Where did this heat come from? For two nights before this, while under the influence of the northerly wind area, and only a fresh to light wind at that, the temperature rose but little during the day and fell during the night more than it would under ordinary conditions. * * * This coldness of the air does not feel as if it were due to long brooding or stagnation over cold

[2] Presented (by title) before the American Meteorological Society at Chicago, Ill., Dec. 29, 1920. Abstract published in *Bull. Am. Met. Soc.*, March, 1921, 2: 40.

surfaces, it has a clean, fresh feel. Then again, in this area of northerly winds, as a rule, how clear the sky is!

In the summer this same condition is present, only, of course, greatly modified and masked by the great power of the sun. Here we have a clear sky and radiation from the sun supreme with every chance for high temperature, but the northerly winds keep the temperature down and but little rise occurs. Under the same conditions and southerly winds a "scorcher" would have been the result. * * *

As a rule * * * as long as the HIGH keeps its form and activity, there is a stationary or falling temperature within this area.—*Douglas F. Manning.*

DISCUSSION.

The "formation" of a HIGH and a big fall in temperature are coincident phenomena, since it is the arrival of an appreciable layer of cold dense air ousting warmer and therefore less dense air that makes the pressure rise so. When a HIGH appears without any change in temperature at the surface having occurred, a considerable body of cold air *must* have arrived aloft. For example, a fall of 5° C. at 500 meters altitude, 10° at 1,000 meters, and 10° at 1,500 meters would increase the density of this layer of air sufficiently to raise the surface pressure about 5 millibars (0.15 inch). Such a fall in temperature aloft would be possible without effect on the surface temperature if at first, as is commonly the case before cold waves in winter, the temperatures up to 1,500 meters were no lower than at the surface. Conversely, at the end of the cold wind, when a warm one sets in aloft, there may be no surface wind or other visible reason for a change, but the pressure will fall, and the air temperature at the surface may rise by the receipt of heat radiated from the warmer wind not far aloft.

Let us consider how the surface air temperature can fall so much faster than radiation, and transportation along the surface, could reduce it. After the temperature at 1 kilometer above the surface has fallen to 10° C. colder than that at the surface any further fall will be accompanied by a convectional interchange and an equal fall in temperature at the surface, the upper wind descending in a sudden gusts and routing upward the masses of warmer air near the surface. The temperature of the wind when it reaches the surface will be about 10° C. higher than when it started down from 1 kilometer. Nevertheless, its temperature will be lower than that which prevailed immediately before the gust arrived from aloft; otherwise it would not have come down. If the surface wind is 15 miles an hour that at 1 kilometer is likely to be 30 miles an hour, although its component from the direction of the surface wind may not be more than 25 miles an hour. This being the case, if the surface wind would in the course of the night lower the temperature at a place 5° C. merely by transportation along the surface, the fall to be expected would be nearly 5° more because of the wind aloft. Stronger winds and radiation from a snow surface into a clear sky at night would make extreme falls in temperature. Therefore, in forecasting the temperature change in a cold wave, the probable transportation of cold air at a moderate height is of more importance than that along the surface.—*Charles F. Brooks.*

BALLOON RACING—A GAME OF PRACTICAL METEOROLOGY.

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By RALPH H. UPSON, Consulting Aeronautical Engineer.

[22 E. 17th St., New York City, Feb. 25, 1921.]

Transportation has always been more or less influenced by the weather. During the last 50 years, this influence has been somewhat lessened by the increasing use of automotive vehicles. Now, however, the advent of commercial air navigation makes weather more of a factor than it has ever been before, so much so indeed that the success of aircraft for commercial use will almost depend on how far the design progress can be paralleled by developments in three-dimensional meteorology. Now the question is: How can we best study this very important factor of weather in its application to aeronautics? Meteorology is no more an exact science than medicine is. To be sure, there *are* laws and principles that can be implicitly relied upon, but the great bulk of our future development for some time to come must depend on the accumulation and coordination of plain *facts experience,* and *practice.* The performance of any aircraft (whether heavier- or lighter-than-air) is a resultant of two factors: (*a*) The power plant of the craft, and (*b*) the surrounding air, or broadly speaking—the weather. Quantitatively, the effect of the weather is usually unchanged by a difference of speed and maneuverability of the aircraft. For example, a 20-mile side wind blows an airplane or airship sideways at just 20 miles per hour regardless of its speed of advance.

As in other branches of science, the best way to study this important subject from a practical standpoint is to separate it as far as possible from outside influences which only disturb the observations and confuse the result. The free balloon is almost ideally suited to our present purpose for the following reasons;

1. Having no motor, its control is entirely dependent on coordination with existing weather conditions. The performance of a balloon is exactly like that of a free particle of air with the addition of altitude control.
2. The entire freedom from pitching, vibration, noise and wind, permits the most delicate observations to be made.
3. Its simplicity and safety of operation makes a balloon especially desirable for a great variety of experiments. A free balloon is so safe that it is *practically* fool proof. It would take considerably more than an *ordinary* fool to hurt himself in one.

The progress in other branches of aeronautics has if anything increased the value of the free balloon for training. During the war all our airship pilots and a large proportion of our kite-balloon observers received preliminary training by free balloon. For training in navigation and meteorology it has also been advocated for airplane pilots. But its greater and broader value lies in the general stimulus to meteorological knowledge to be gained by its development as a recognized sport. And it is in no disparagement of its scientific and practical importance to say that as a sport, ballooning is the finest that can be imagined. It is also very moderate in cost. Recent developments in fabric and gas generators put ballooning within reach, financially and otherwise, of any moderate-sized club.

The *safe* piloting of a free balloon is easily and quickly learned. There are only two controls, ballast and gas. To go up or stop coming down, throw out ballast. To come down or stop going up, let out gas. The control of altitude and with it the choice of the desired wind cur-